LETTERS TO THE EDITOR

Regarding "Mandibular subluxation stabilized by mouthpiece for distal internal carotid artery exposure in carotid endarterectomy"

We read with interest the report by Yoshino et al[1] of their technique for achieving mandibular subluxation for distal internal carotid artery (ICA) exposure. The indications for extended carotid exposure are well established, and mandibular subluxation is a minimally invasive procedure to gain 15 to 20 mm additional exposure of the ICA. The authors advocate the use of a custom-made resin mouthpiece. The possible advantages of this method would be reduced oral injury and infection, avoidance of dislocation of the temporomandibular joint (TMJ) and short preparation time. We recently reported in the *Journal of Vascular Surgery* on the use of ipsilateral monocortical miniscrews and wiring for this indication[2] with the same advantages as stated in their article. We do not recognize the described injury and infection to the oral cavity in our routine. These complications are extremely rare due to the excellent blood support to the alveolar ridge and the minimally invasive nature of miniscrews. Only in cases of an extremely resorbed mandibula could wound healing disturbances theoretically occur.

Second, there is the issue of the practical applicability and stability of the described resin mouthpiece. It was possible to produce a mouthpiece in 57% of the described patients. In our population, this amount would be even lower due to periodontal disease and absence of dentition. Next to the issue of the presence of an adequate dentition, it would be interesting if the authors describe whether the resin mouthpiece has enough stability and retention on the existing dentition to withstand the muscular retraction forces during operation, which can reduce the amount of subluxation and subsequently the exposure of the ICA.

A third possible disadvantage of this technique is the thickness of the mouthpiece. Interposition of resin material between the dental arches causes opening of the bite and movement of the mandibular angle in a backward direction over the desired exposed area of the ICA. Therefore, we feel it should be desired to produce a position of maximum flexion in the TMJ. The suggestion that the production of a mouthpiece of mandibular subluxation in an awake situation will produce fewer disturbances to the TMJ than performing this in general anesthesia is unsubstantiated. In our experience, mandibular subluxation without dislocation of the TMJ can easily be achieved in the experienced hands of a dentist or maxillofacial surgeon. We have not had the experience that patients suffered from postoperative TMJ complaints. We do see that the extension of the subluxation under general anesthesia is greater that in an awake patient, which gives a better exposure of the ICA.

In summary, the suggested benefits of a resin mouthpiece over the use of monocortical screws may be questioned. It is more expensive, time-consuming in preparation, and, in our opinion, not as reliable as using monocortical miniscrews and wiring.

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Reply

We thank Dr Jaspers et al for their interest in our article[1] and are happy to reply to their comments:

First, although their technique provides the same advantages as our technique for exposure of the operative field, we think that the technique carries potential risks, such as bleeding, infection, and dental injury, because the insertion of screws is an invasive procedure. They report no such complications, but in only four cases.[2] Even if such complications are extremely rare, it is better to avoid invasive procedures.

Second, it is true that our technique cannot be used for edentulous patients, and this is an important disadvantage. In our experience, the mouthpiece provides adequate stability, has never fallen back during surgery, and has never interfered with the operative field.

Third, our illustrations exaggerate the resin material between the dental arches for easier interpretation, but as shown in our photographs, the thickness of the resin is only about 2 mm. Therefore, the displacement of the mandibular angle in the backward direction is very small and has a very small effect during the operation.

We know that extension of the subluxation under general anesthesia is greater than that in an awake patient, but in our experience, the exposure of the internal carotid artery is rarely different between these conditions. Therefore, we recommend our technique because it is less likely to cause dislocation of the temporomandibular joint. It is possible that an experienced dentist or maxillofacial surgeon can achieve adequate mandibular subluxation without dislocation, but not all dentists and maxillofacial surgeons have the requisite skills or experience. This type of mouthpiece is commonly used for the treatment of temporomandibular disorders, so any dentist ought to be able to perform our technique with the information included in this article.

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Regarding "Infrapopliteal balloon angioplasty for the treatment of chronic occlusive disease"

Critical limb ischemia (CLI) usually represents extensive multilevel arterial occlusive disease, often requiring infrapopliteal revascularization. Angioplasty also seems very promising in this segment. Previous studies on the effect of infrapopliteal angioplasty in CLI were often limited by a lack of description of patient and lesion characteristics. In a recent article in the *Journal of Vascular Surgery,* Conrad et al[1] described their results with infrapopliteal angioplasty in a large study with 144 patients and with well-described CLI and lesion characteristics. The limb salvage rate at 40 months of follow-up was 86%, despite inferior patency rates. The authors concluded that infrapopliteal angioplasty should be considered the initial therapy for these patients.

Important information is missing in their report, however. Almost 70% of their patients had diabetes mellitus, and claiming a success in limb salvage as a result of angioplasty in such patients seems somehow inappropriate. Tissue loss and healing of ulcers in diabetic patients is strongly related to diabetic neuropathy, infections, and microvascular diabetic complications, besides peripheral arterial occlusive disease.[2] Many of these patients often have an ankle-brachial index of about 50 mm Hg or higher (subcritical ischemia), and limb loss rates are relatively low whether they undergo revascularization or not.[3,4] Information on the presence of neuropathy and other microvascular complications, as well as the actual ankle-brachial index, are needed to adequately interpret the results reported by Conrad et al.

Furthermore, multilevel treatment was necessary in 74% of their patients. Dilatation of inflow lesions was performed in 40% to 88% of the patients in infrapopliteal angioplasty studies. Experience with combined multisegment occlusive disease indicates that treatment of the more proximal lesion alone was appropriate in >40% to 75% of the patients to relieve CLI.[5] The concurrent angioplasty of inflow lesions may explain the observed high gap between infrapopliteal lesion patency and limb salvage rates. What were the proximal lesions (ie, TransAtlantic InterSociety Consensus classification and level), what procedures were performed for these more proximal lesions (angioplasty or bypass surgery), and what were the patency rates of these procedures in the Conrad et al study? This information is needed to understand their success in treating these patients.

Finally, indicating angioplasty as the initial therapy in patients with CLI needs some caution after the results of the Bypass Versus Angioplasty in Severe Ischaemia of the Leg (BASIL) trial.[6] Approximately 75% of the BASIL cohort survived >2 years, and an angioplasty-first strategy in these patients did not fare as well as a bypass surgery-first strategy.

Improvements in endovascular therapy allow an increasing subset of patients to be treated with angioplasty. Angioplasty and open surgery are, however, complementary, and therapy must be individualized. More information is needed from Conrad et al to fully appreciate their study.

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Reply

We respectfully disagree with Dr Meerwaldt and colleagues' assertion that diabetic patients with critical limb ischemia will heal ulcers without revascularization. Although it is true that diabetic wounds are often more difficult to manage due to factors such as neuropathy, infection, and microvascular disease, in the absence of macrovascular blood flow, these wounds simply do not heal. The current study did not intend to address the nuances of management of the diabetic foot and indeed, many of our diabetic patients (ie, those with intact macrovasculature) were not included in this series.

In our practice, we attempt to re-establish in-line flow to the foot in patients with tissue loss, and although improvement of inflow alone will often relieve rest pain, this approach is usually inadequate for ulcer healing. The disconnect between the primary patency and limb salvage in the current series is secondary to a strict definition of failure and an aggressive posture toward reintervention, as was stated in the article.

Although the Bypass Versus Angioplasty in Severe Ischaemia of the Leg (BASIL) trial has shown excellent results with its primary end points of survival and limb salvage, it is not applicable to the current series. Patients in BASIL were included only if there was agreement that they could be treated with angioplasty or bypass. In the current series, many of our patients were not bypass candidates due to comorbidities or a lack of an autogenous conduit. In addition, the difference between the two cohorts is accentuated by our 24-month survival of 68% (lower in those with critical limb ischemia) compared with a higher rate in the BASIL series.

Finally, we agree with Dr Meerwaldt and colleagues' statement that lower extremity revascularization needs to be tailored to the individual patient and stand by our results as written.

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